Though it has been remarked, that a calm always attends an earthquake; no ſuch thing happened now, a fine gale of wind blowing before and after, as well as during the time of, the ſhock.

The ſky was ſerene, interſperſed with flying clouds.

The ſun, which ſhone very bright, immediately after the earthquake was ſurrounded by a very large halo, which laſted about an hour, and gradually diſappeared.

XXXI. *An Account of a Treatiſe in* Latin, *preſented to the Royal Society, intituled,* De admirando frigore artificiali, quo mercurius eſt congelatus, diſſertatio, &c. a J. A. Braunio, Academiæ Scientiarum Membro, &c. *by* William Watſon, *M. D. R. S. S.*

To the Royal Society.

Gentlemen,

Read May 21, 1761.

VERY early laſt year, we were informed, that at Peterſburg, by the means of artificial cold, the mercury in thermometers had been condenſed to ſo great a degree, as to become perfectly fixed and ſolid: but as this information was received only in a looſe way, from the public gazettes, the opinions of philoſophers here were ſuſpended, in relation to their giving credit to

this very extraordinary phænomenon, until the truth of it could be ſufficiently authenticated. This has very lately been done by Profeſſor Braun, who firſt made the experiments, and who preſented an account of them to the Royal Academy at Peterſburg, a printed copy of which has been communicated by him to the Royal Society.

Profeſſor Braun obſerves, that every age has its inventions, and that the diſcovery of ſome things ſeem to be reſerved for particular perſons. To this, the hiſtory of ſciences in all ages, more particularly of the late and the preſent, bears witneſs ſufficiently, by the invention of the air-pump, barometers, thermometers, optical inſtruments, electricity, more particularly the natural, artificial magnets, phoſphorus, the diſcovery of the aberration of light, and of many other things in natural philoſophy. He does not know, whether the congelation of mercury, which it was his good fortune to diſcover, may not be ranged among theſe: for who did not conſider quickſilver, as a body, which would preſerve its fluidity in every degree of cold? Neither was the fact otherwiſe, if this is underſtood of natural cold, ſuch as it has been found in any part of the globe, hitherto diſcovered. But if it ſhould happen, that the natural cold ſhould ever be ſo intenſe as artificial cold has been found to be, the whole globe would have a different face, as men, animals, and plants, would certainly be deſtroyed. He did hint ſome time ſince, in a diſſertation upon the degrees of heat, which certain liquors and certain fluids would bear before they boiled, and the degrees of cold they reſpectively bore, before they were converted into ice, that there was a ſuſpicion, that the

mercury

mercury in ſome of the barometers and thermometers made uſe of for experiments in Siberia had been frozen: but ſince that in greater degrees of cold, the mercury continued fluid in other barometers and thermometers, the immobility and hardneſs obſerved in ſome of theſe inſtruments, was attributed more probably to the lead or the biſmuth, with which the mercury had been adulterated, and was not conſidered as a real freezing of the mercury: but this has been ſince put out of all doubt; ſince it is certain, that pure mercury would not freeze under ſuch ſmall degrees of cold, great as they were for natural cold. The experiments, which the profeſſor made, in order to congeal mercury, demonſtrate this moſt evidently; beſides which, they exhibit new phænomena.

There happened at Peterſburg, on the 14th of December 1759, a very great froſt, equal if not more intenſe than any which had been obſerved there: for, between nine and ten o'clock in the morning, Deliſle's thermometer ſtood at 205; at ſeven o'clock, at 201; which laſt was the greateſt degree of cold, that had been obſerved at Peterſburg, either by himſelf or others. At one o'clock at noon, the thermometer ſtood at 197. Mr. Braun had been employed, ſeveral days before this, in obſerving the ſeveral degrees of cold, which different fluids would bear, before they were converted into ice; partly to confirm thoſe things which he had already laid before the academy; and partly to make experiments upon liquors, which had not yet been examined; as on the days between the 7th and 14th the cold was intenſe

intenſe enough to be between the degrees of 181 and 191.

When the natural cold was ſo intenſe as to be at 205, Profeſſor Braun conjectured, that it was of all others the moſt proper occaſion to try the effects of artificial cold; not doubting, but that artificial cold would be increaſed in proportion as the natural was more intenſe. Aquafortis, which was found by the thermometer to be 204 degrees cold, was the greateſt part of it frozen, the ice having the appearance of cryſtals of nitre; which, however, immediately diſſolved in a ſmall degree of heat. This aquafortis, which though frozen at the ſides, was liquid in the middle, was poured upon pounded ice, in that proportion which was directed by Fahrenheit, the firſt perſon who made artificial cold with ſpirit of nitre. But before the profeſſor made this experiment, he, by examination, found, that both the ice and aquafortis were of the temperature with the air, which was then 204. Upon the firſt pouring, the mercury fell 20 degrees; this ſpirit was poured off, and freſh put on, ſeveral times; but it was poſſible, by theſe means, to introduce no more than 30 degrees of cold; ſo that the mercury in the thermometer fell no lower than 234. Since therefore Fahrenheit could not produce cold greater than that of 40 below the cypher of his thermometer, which correſponds with 210 of that employed by Profeſſor Braun; nor Reaumur, nor Muſchenbroek, who often repeated the ſame experiment, our author was upon the point of giving up this purſuit; as conſidering this as the greateſt degree to which artificial cold could be carried; thinking it ſufficient honour to himſelf,

to

to have added 20 degrees to the cold formerly known.

But reflecting, that this was not all the fruit he expected from these experiments, he determined to pursue them; but at the same time, however, to vary the manner of them. By good fortune, his ice was all gone, and he was compelled to use snow in its stead, after having first tried, and found the snow of the same degree of cold with the air, at this time 203. The snow, the thermometer, and the aquafortis, being of the same temperature, he immersed the thermometer in snow, contained in a glass; and, at first, only poured a few drops of the aquafortis upon that part of the snow, in which the thermometer was immersed; upon which he observed the mercury to subside to 260. Elated by this remarkable success, he immediately conceived hopes, that these experiments might be carried further: nor was he deceived in his expectations; for repeating the experiment in the same simple manner, he poured on only some more aquafortis, and immediately the mercury fell to 380. Upon which he immersed the thermometer in another glass filled with snow, before it had lost any of this acquired cold; and at length, by this third experiment, the mercury subsided to 470 degrees. When he observed this enormous degree of cold, he could scarce give credit to his eyes, and believed his thermometer broke. But, to his infinite satisfaction, upon taking out his thermometer, he found it whole; though the mercury was immoveable, and continued so in the open air twelve minutes. He carried his thermometer into a chamber, where the temperature of the air was 125 degrees;

grees; and, after ſome minutes, the mercury being reſtored to its fluidity, began to riſe. But to be certain, whether this thermometer had received any injury, and whether it would yet correſpond with his thermometer, which he keeps as a ſtandard, he ſuſpended them together, and in twenty minutes the thermometers correſponded one with the other.

The thermometers, which our author uſually employs, have a ſpherical bulb, and their ſcale is divided into 1200 parts, of which 600 are above the cypher, which denotes the heat of boiling water, and 600 below that heat. A thermometer of this conſtruction was uſed in inveſtigating the heat of boiling mercury and oils. He had another thermometer, of which the ſcale went no lower than 360 degrees below the cypher, denoting the heat of boiling water. He repeated the former experiment with this, and the mercury very ſoon deſcended ſo, that the whole was contained in the bulb, which, however, it did not quite fill. The mercury in this bulb was immoveable, even though he ſhook the thermometer; until about a quarter of an hour, it began to aſcend in the open air; and it continued to aſcend, till it became higher than the circumambient air ſeemed to indicate. He was ſtruck with this extraordinary phænomenon, and very attentively looked at the mercury in this thermometer, and found certain air bubbles interſperſed with the mercury, which were not in that of the other thermometer. From theſe, and other experiments (it would be unneceſſary to recite them all), he was ſatisfied, that the mercury in theſe thermometers had been fixed and congealed by the cold.

Hitherto our profeſſor had only ſeen the mercury fixed within the bulb of his thermometers. Theſe he was unwilling to break. He was, however, deſirous of examining the mercury in its fixed ſtate, and therefore determined to break his thermometers in the next experiments. It was ſeveral days before he got other thermometers, which exactly correſponded with thoſe he had already employed.

When theſe were procured, the natural cold had ſomewhat relented. In the former experiment, the thermometer ſtood at 204; it was now at 199. In making the experiment, he varied the manner a little. He firſt put the bulb of the thermometer into a glaſs of ſnow, gently preſſed down, before he poured on the aquafortis; he then, in another glaſs, poured the aquafortis upon the ſnow, before he immerſed his thermometer therein; he then, in like manner, put the ſnow to the aquafortis, before he put his thermometer therein. Which ever of theſe ways he proceeded, he found the event exactly the ſame; as the whole depended upon the aquafortis diſſolving the ſnow. When he had proceeded ſo far, as to find the mercury immoveable, he broke the bulb of the thermometer, which had already been cracked in the experiment, but the parts were not ſeparated. He found the mercury ſolid, but not wholly ſo, as the middle part of the ſphere was not yet fixed. The external convex ſurface of the mercury was perfectly ſmooth; but the internal concave one, after the ſmall portion of mercury, which remained fluid, was poured out, appeared rough and uneven, as though compoſed of ſmall globules. He gave the mercury ſeveral ſtrokes with the peſtle of a mortar, which ſtood

near

near him. It had ſolidity enough to bear extenſion with theſe ſtrokes; its hardneſs was like that of lead, though ſomewhat ſofter; and, upon ſtriking, it ſounded like lead. When the mercury was extended by theſe ſtrokes, he cut it eaſily with a penknife. The mercury then becoming ſofter by degrees, in about twelve minutes it recovered its former fluidity, the air being then 197. The colour of the congealed mercury did ſcarce differ from that of the fluid: it looked like the moſt poliſhed ſilver, as well in its convex part, as where it was cut.

The next day, the cold had increaſed to 212 degrees, which was 7 degrees beyond what it had ever before been obſerved at Peterſburg. The ſeaſon ſo much favouring, he thought it right to continue his purſuit, not only in further confirmation of what he had already obſerved, but to inveſtigate new phænomena. In two thermometers, he obſerved the ſame facts in relation to the congealing of mercury, as he did the preceding day. In the bulbs which he broke, the whole of the mercury was not fixed, as a very ſmall portion, much leſs than that of the preceding day, continued fluid. He treated this mercury as he did the former; he beat it with a peſtle, he cut it, and every thing was thus far the ſame. But he ſaw a very great difference in relation to the deſcending of the mercury in the thermometer, the like of which did not occur to him, neither in the former nor any of the ſubſequent experiments. From the former ones it appeared, that the mercury in the firſt experiment had only deſcended to 470, when it became immoveable, though the glaſs bulb was not cracked. In the experiment of the 25th, it deſcended

to 530; and in two thermometers on the 26th, to 650. But as well in the thermometer, which he uſed on the 25th, as in two of the 26th, the bulbs were cracked in the experiment: they cohered however; nor was the leaſt part of the bulb ſeparated, but the congealed mercury ſeemed to adhere to all parts of the bulb. In the following experiments, he invariably found, that the mercury ſunk lower, if the whole of it was congealed, than if any part of it remained fluid. It then generally deſcended to 680 and 700, but the bulbs were never without cracks; moreover, it deſcended to 800, and beyond even to 1500; but in this laſt experiment, the bulb was quite broke, ſo that the globe of mercury, thoroughly frozen, fell out, and by its fall, of about 3 feet, the globe of mercury became a little compreſſed; but in the former, only ſome parts of the bulb fell off.

Mr. Braun always found, that, cæteris paribus, the more intenſe the natural cold was, the more eaſy and more expeditiouſly theſe experiments did ſucceed.

In continuing theſe experiments he obſerved, that double aquafortis was more effectual than ſimple ſpirit of nitre; but that if both the aquafortis and Glauber's ſpirit of nitre, which he ſometimes alſo uſed, were well prepared, the difference was not very conſiderable. When his aquafortis was frozen, which often happened, he found the ſame effects from the frozen parts, when thawed, as from that part of it, which remained fluid in the middle of the bottle. Simple ſpirit of nitre, though it ſeldom brought the mercury lower than 300 degrees, by the following method he even froze mercury with it.

He

He filled fix glaffes with fnow, as ufual, and put the thermometer in one of them, pouring thereupon the fpirit of nitre. When the mercury would fall no lower in this, he, in the fame manner, put it in a fecond, then in a third, and fo in a fourth; in which fourth immerfion, the mercury was congealed.

Another very confiderable difference prefented itfelf in purfuing thefe inquiries, with regard to the mode of defcent of the mercury. He conftantly and invariably obferved, that the mercury defcended at firft gently, but afterwards very rapidly. But the point, at which this impetus begins, is not eafy to afcertain; as in different experiments it begins very differently, and fometimes at about 300, at other times about 350, and even further. In the experiment before-mentioned, in which the mercury fell to 800, it proceeded very regularly to 600; about which point it began to defcend, with very great fwiftnefs, and the bulb of the thermometer was broke. The mercury, however, was perfectly congealed.

He frequently obferved another remarkable phænomenon; which was, that although the fpirit of nitre, the fnow, and the mercury in the thermometer, were previoufly reduced to the fame temperature, upon pouring the fpirit of nitre upon the fnow, the mercury in the thermometer rofe. But as this did not always happen, he carefully attended to every circumftance; from which it appeared, that this effect arofe from his pouring the aquafortis immediately upon the bulb of the thermometer, not previoufly well immerfed in the fnow. He likewife obferved another effect, twice only; and this was, that, after

the

the thermometer had been taken out of the ſnow and aquafortis, the mercury continued to ſubſide, in the open air, down as low as the congealation of mercury.

In the courſe of theſe inquiries, our profeſſor found no difference, whether he made uſe of long or ſhort thermometers; whether the tubes were made of the Bohemian, or the glaſs of Peterſburg. Under the ſame circumſtances, the ſame effects were always produced, making an allowance for the different contraction of the different glaſſes, under ſo ſevere a degree of cold. But if theſe tubes were filled with different mercury, there was then a ſenſible difference; inaſmuch as mercury revived from ſublimate did not ſubſide ſo faſt in the thermometer, as that did, which was leſs pure. He has even found, that he has been able to congeal the leſs pure mercury, at a time when he could not bring the revived mercury lower than 300 degrees: but this he would, till farther trials have been made, not have conſidered as a general axiom.

From theſe experiments, our author conceives it demonſtrated, that heat alone is the cauſe of the fluidity of mercury, as it is that of water and other fluids. If, therefore, any part of the world does exiſt, in which ſo great a degree of cold prevails, as to make mercury ſolid, there is no doubt, but that mercury ought to appear there as a body equally firm and conſiſtent, as the reſt of the metals do here: that mercury, upon congealing, becomes its own ice, however different the mercurial ice may be from that of water, or other liquids. The idea of freezing does or can comprehend nothing more than

than a tranſition of bodies from a ſtate of fluidity to that of firmneſs by the ſole interpoſition of cold.

The ice of oily and ſaline bodies differs greatly from that of water, which is friable and eaſily broke, whereas that of mercury is ductile. And M. Braun proceeds to conſider all bodies, which liquify by heat, as ſo many ſpecies of ice; ſo that every metal, wax, tallow, and glaſs, comes within his view, in this reſpect.

Mercury then is, in its natural ſtate, a ſolid metal; but is fuſible in a very ſmall degree of heat. Every metal begins to flow in a certain degree of heat; but this degree is different in different metals. Pure tin begins to run at 420; lead, at 530; and biſmuth, at 470, in Fahrenheit's thermometer: or, according to our author, lead liquifies at 320 above the cypher in his ſcale, which correſponds with 596 in Fahrenheit; lead at 170 = 416 of Fahrenheit; biſmuth at 235 = 494; zinc requires a greater heat to melt it than will make mercury boil. Now, if it could be ſettled, at what point mercury would begin to be congealed, we ſhould know the point at which it began to flow; as it has been long known, that water is either fluid or ſolid, as the heat of it is a very few degrees above or under 32 in Fahrenheit's thermometer. Juſt ſo metals become ſolid, at almoſt the ſame degree of heat in which they become fluid. But in mercury, the congealing point is at too great a latitude to be exactly determined; but our author eſtimates it to be about 469 degrees in his thermometer; at a leſs degree than which, he has not been able to obſerve the ſlighteſt congealation. Hence it follows, that the condenſation or contraction, and

conſequently

consequently the diminution of the volume of mercury must be very great indeed. This is demonstrated by the great descent of the mercury in the thermometer, while it is freezing. But how great this diminution of the volume of the mercury is, cannot exactly be determined; and hence arises no small difficulty in determining its specific gravity, as this last must increase, as the bulk of the mercury lessens. Hence as mercury, even in its fluid state, comes of all bodies, platina excepted, the nearest to gold; in its solid state, it must still approach much nearer.

Our author had three thermometers filled with the most highly rectified spirit of wine. These not only corresponded exactly with one another, but, in less severe trials, corresponded reasonably well with those filled with mercury. But by the mixture of snow and spirit of nitre, which froze the mercury, he never was able to bring the spirit thermometers lower than 300. From hence it appears, that the heat, which will freeze mercury, will not freeze spirit of wine; and that therefore spirit thermometers are the most fit to determine the degree of coldness in frigorific mixtures, until we are in a situation to construct solid metallic thermometers with sufficient accuracy.

Our author made many experiments, to try the effects of different fluids, in his frigorific mixtures. He invariably found, that Glauber's spirit of nitre and double aquafortis were the most powerful. With oil of vitriol, the most ponderous of all acids, he was never able to congeal mercury. He likewise tried a great number of other fluids, both acid and spirituous, which though, when mixed with snow, produced cold, it was in very different degrees. He

tried

tried a ſeries of experiments to this purpoſe; but it was in weather far leſs cold than the preceding experiments were tried in, viz. between 159 and 153, by his thermometer. By theſe it appears, that ſpirit of ſalt pounded upon ſnow, increaſed the natural cold 30 degrees; ſpirit of ſal ammoniac, 10; oil of vitriol, 35; Glauber's ſpirit of nitre, 58; aquafortis, 40; ſimple ſpirit of nitre, 30; ſpirit of vinegar, and lemon juice, made no remarkable difference; dulcified ſpirit of vitriol, 20; Hoffman's liquor anodynus, 32; ſpirit of hartſhorn, 10; ſpirit of ſulphur, 10; ſpirit of wine rectified, 20; camphorated ſpirit, 15; French brandy, 12; and even ſeveral kinds of wine, increaſed the natural cold to 6, 7, or 8 degrees. That inflammable ſpirits ſhould produce cold, ſeems very extraordinary, as rectified ſpirit ſeems to be liquid fire itſelf; and what ſtill appears more pardoxical is, that inflammable ſpirits poured into water, cauſe heat; upon ſnow, cold: and what is water, but melted ſnow?

Though not immediately relating to the principal purpoſe of this treatiſe, our author meaſured by his thermometer, when it ſtood in his ſtudy at 128 degrees, the heat occaſioned by pouring different fluids into water. He found, that oil of vitriol produced 35 degrees; ſpirit of ſea ſalt, 10; Hoffman's anodyne liquor rectified, 5; ſpirit of wine, 10. On the contrary, ſpirit of ſal ammoniac mixed with ſnow, ſpirit of ſulphur, and ſpirit of hartſhorn, mixed likewiſe with ſnow, made no perceptible difference. Highly rectified chymical oils, mixed with water, produced no heat; nor with ſnow, no cold; as was tried in the oils of turpentine, amber, mint, and mother of thyme.

 And

And here it is to be remarked, notwithſtanding the contrary has been given out by ſome, that theſe chymical oils mixed with the moſt highly rectified ſpirit of wine, do produce no cold, either upon their mixture, or half an hour after.

It reſults from theſe experiments, that although there are many liquids, which can produce artificial cold, the nitrous acid is the moſt powerful; and mercury may be congealed by it, without any difficult proceſs, at any time, when the heat of the atmoſphere is not greater than 175 by the thermometer before-mentioned. And theſe experiments have not only ſucceeded with our author, but with many others; among whom, it may be ſufficient to mention Meſſieurs Lomonoſow, Zeiher, Aepinus, and Model, as theſe gentlemen have made themſelves well known in the philoſophical world. The nitrous acid was poured upon the ſnow, in no determinate quantity; ſometimes a few drops were ſufficient, ſometimes it required a larger quantity. Snow ſeems to be more fit for thoſe experiments, than pounded ice; as the former, from its looſe texture, is of more apt and eaſy ſolution.

Hence it appears, that mercury is no longer to be ranked with the ſemi-metals, but as a perfect one, fuſible, though with a much leſs degree of heat than any of the others. It agrees likewiſe with other metals; as their parts like it, when in fuſion, attract one another, and run into globules, and, from a ſtate of fluidity, paſs into a ſolid ſtate, not all at once, but ſucceſſively, and vice verſâ. But it is worth inquiring, whether this metal, which agrees with all others, both in a ſolid and fluid ſtate, has not the particular

property of boiling at a certain degree of heat, which is by no means to be obferved in other metals. The degree of heat, in which mercury begins to boil, is not at 600 of Fahrenheit's fcale, as is generally imagined; but at leaft at 709 of the fame fcale, which correfponds with 414 of our author's, whofe cypher is at the heat of boiling water.

Both the boiling and freezing of mercury have this in common; that when it begins to boil, it rifes with rapidity;. and defcends rapidly, when it begins to freeze. If, therefore, the mean term of the congealation of mercury is fixed at 650 below the cypher, and the term of its boiling at 414 above the cypher; its greateft contraction to its greateft dilatation, will be 1064 degrees of our author's thermometer, and 1237 of Fahrenheit's; as 212 is the point of boiling water in this laft, and 32 the freezing one; which correfponds with 150, under the term of boiling water, in our author's. Hence every one will fee the great alteration of fpecific gravity in frozen and boiling mercury, as, between one and the other, the tenth part of the volume is leffened.

It may be afked, why the mixture of fnow and niturous acid does not run into a folid mafs, and form itfelf into ice, but remain of a foft confiftence, although actually much colder, than what is required to freeze aquafortis? We have already mentioned, that aquafortis freezes at 204 of our author's thermometer, which correfponds with 34 below the cypher of Fahrenheit's. The frigorific mafs, in a degree of cold far below this, remained foft like a pultice. The caufe of this extraordinary phænomenon feems to be no other than a continuation of the folution of

the ſnow, and its mixing with the nitrous acid. For as the production of cold depends ſolely upon the ſolution and mixture, it cannot happen, that this maſs, which conſtitutes a fluid of a hard kind, ſhould run into a ſolid conſiſtence, ſo long as the ſolution and mixture continue.

And now, Gentlemen, it requires no ſmall ſhare of your indulgence, to pardon my having extended this account ſo far: but I have to plead in my excuſe, that the ſubject of this work is intirely new, and replete with a vaſt variety of curious facts; all which exactly fall in with our excellent inſtitution. For who, before Mr. Braun's diſcovery, would have ventured to affirm mercury to be a malleable metal? who, that ſo intenſe a degree of cold could be produced by any means? who, that the effects of pouring nitrous acid upon ſnow, ſhould ſo far exceed thoſe, which reſult from mixing it with ice; when ſnow and ice are produced from the ſame ſubſtance, and ſeem to differ only in their configuration? As Mr. Braun's work is in very few hands, I had reaſon to hope, that you would not be diſpleaſed to be informed, in a degree ſomewhat circumſtantial, of theſe very extraordinary facts.

I am,

With the moſt profound reſpect,

Gentlemen,

Your moſt obedient,

humble ſervant,

April 18, 1761. W. Watſon.

XXXII. *Ob-*